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ABSTRACT

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In practice, hydrology is regarded mostly as a technological discipline rather than a science; this attitude is responsible for much bad science in hydrology which, in turn, has led to much bad technology in applied disciplines. It is argued that the present offers a unique opportunity to fill the vacant niche of the science of hydrology and some suggestions for action are offered.

The paper was written in the author's capacity as the current President of the International Association of Hydrological Sciences.

INTRODUCTION

Hydrology is in a situation where perhaps 80% of its activities can best be described in the laconic idiom of the famous jokes purporting to be answers of Radio Yerevan to questions on various controversial issues:

Question: Is it true that hydrologists are the scientists who study the relationships within the water cycle?

Answer: In principle yes, but: they are not scientists — they are technologists; and they don't study them — they fudge them.

This theme was elaborated in much greater detail in Klemeš (1986) and its echo can be heard, for example, in Bras and Eagleson (1987). Commenting on hydrology as one of the geophysical sciences they say: "... in the modern science establishment, this niche is vacant".

In this paper the problems of hydrology are placed in a broader context in an attempt to sharpen the diagnosis and suggest a therapy. Also, it is argued that due to a unique historical coincidence, we have entered a relatively short period which is extremely favourable for starting an effective treatment, and that such an opportunity may not be repeated.

SCIENCE AND TECHNOLOGY, RESEARCH AND DEVELOPMENT

The problems of hydrology can be seen most clearly when placed in the context of a more general framework circumscribed by the relationships among

science, technology, research and development. However, it may be useful first to define the meaning in which these terms will be used here since, while they are household words, their usage has become so loose that it precludes meaningful communication and is good only for the jargon of government boardrooms and the journalese of popular magazines. Their definitions given below are consistent with those given in dictionaries, e.g. the Random House Dictionary (Stein, 1967).

Science: The body of organized knowledge. The mission of science is to understand things.

Technology: The ways in which society provides itself with the material objects of its civilization. There is an implication that technology based on science is superior to one which is not. The mission of technology is to do things.

Research: Extension of knowledge by discovery of new facts, relations, laws, i.e. advancement of science.

Development: Creation of technology, with an emphasis on applying science, i.e. making the existing knowledge useful.

The thesis underlying the present discussion is that science and technology have different missions and that a successful accomplishment of these missions dictates a different mode of operation for each of them. The basic activity for advancing science is research while that for advancing technology is development. In other words, the business of the scientist is to do research, to seek the truth, while the business of the technologist is to do development, to make the established truths work for the benefit of man. Both these activities are indispensable for the advancement of civilization; they are highly interdependent and stimulate each other. Both require high levels of training and high degrees of ingenuity which, however, must be pointed in different directions.

In epistemology, pure knowledge is commonly ranked higher than applied knowledge, *thinking* higher than *doing*. This has been accepted quite generally and is, for example, reflected even in the scientific terminology describing the evolution of the human species in which the growth of the brain volume is identified with an improved ability to think. The label *Homo habilis* — Man the Doer — is used to describe a much lower evolutionary state than *Homo sapiens* — Man the Thinker. Odes on thinking and knowledge abound — and so do condemnations of man's doings. This hierarchy, if meaningful at all, is true only in the historic sense. However, in a given epoch and at a given time the two activities, rather than forming a hierarchy, are locked in a closed loop. *Doing* stimulates *thinking* as much as *thinking* inspires *doing*.

In every time and age, the achievements of science are very much a function of the contemporary state of technology. Astronomy has progressed through the development of ever more powerful telescopes; progress in particle physics depends on the ability to generate ever higher energies; Gutenberg's technological development gave a greater stimulus to science than the thinking of many a researcher, etc.

The confusion about the respective missions of, and relationships between, science and technology, research and development, are aggravated by the

common practice whereby the managements of science and technology are lumped together and done by the same people. Not surprisingly, development is then called research, and the productivity of scientists is often measured by the "usefulness" of their work, forgetting that their true usefulness lies in preparing the knowledge base for enhancing the usefulness of future technologists. This is not to say that one and the same person cannot manage, or do, research as well as development. But it is rare that one could perform at a high level of effectiveness in both capacities and even rarer that one would have enough self-discipline to prevent one mode of operation from dominating the other. The ingenuity of the technologist is directed toward making things work; that of the scientist is invested in solving nature's puzzles. The technologist will tend to disregard an outlier, the scientist will tend to concentrate on it. It is extremely difficult for the same human mind to function effectively and simultaneously in both these modes.

HYDROLOGY: A SCIENCE OR A TECHNOLOGY?

To ask such a question invites suspicion that the author does not know what he is talking about. For even an American high school student who thought that "the main language in Latin America is Latin", vaguely remembered that hydrology is "some science about water" (quoted from memory, from a survey published in a Canadian newspaper about two years ago). Moving onto a safer ground but without belaboring the details, the problem can be settled by consulting, for instance, the Pocket Oxford Dictionary (Sykes, 1978): "*hydrology* — science of the properties of water, especially of its movement in relation to land". It then follows that hydrologists are scientists and that to practice hydrology means contributing to this science, i.e., to use the terminology of the preceding section, to do research — presumably research on the various aspects of the water cycle.

However it may be in other sciences — in biology, physics, geology, etc., in hydrology this logic does not get one very far. It may not even be self-evident that hydrology is "about water", and here I am not talking about an American high school student. For example, one of the editors of this journal likes to recall his attendance of a defence of a hydrological thesis of a doctoral candidate who impressed him very much. The climax of his story is this: "I told the committee that I had nothing against awarding the degree to the candidate but that they should explain to him that floods are caused by rainfall or snowmelt, not by an excess of random numbers".

Such an experience is not unique in hydrology. Quite the contrary, perhaps more than three quarters of all work done under the label of hydrology, including the vast volume of research, textbooks, handbooks, and university courses, is not hydrology at all; it is a motley of attempts to solve hydrology-related technological problems in the absence of hydrological knowledge. This may seem pathological but it has a very simple explanation: Despite its clear definition as a science, in most cases hydrology is still viewed and practised as

a technology. Its mission is not seen as improving the understanding of the water cycle, but as solving the many urgent practical problems involving water, by any means available, including baseflow separation, unlikely results of maximum likelihood estimates, random numbers, and worse.

To solve the problems of flood protection, water supply, irrigation, etc., is of course the responsibility of engineers, water resource managers and other professionals in water-related technologies. It is true that these technologies gave birth to hydrology, as many other technologies stimulated the formation of many other sciences. But it is also true that the thinking in the water-related technologies has not yet reached a state necessary for recognizing hydrology as a science in its own right. In the technological environment, such a development of hydrology is seen as a loss of its focus and relevance, a drift into an ivory tower. Hydrology is the technologists' discipline and it must solve their problems. Who will solve the problems of hydrology? Well, have not most of them been solved a long time ago? As we shall see later, there is a widespread opinion that this indeed is so.

An example of the thinking outlined above appears, for instance, in a recent information brochure on an occupation designated as Hydrologist (Associate Committee on Hydrology (ACH) of the National Research Council of Canada, 1987). It starts with two short paragraphs, one giving a concise and accurate definition of hydrology as a science, the other justifying the evolution of hydrology from an adjunct of other disciplines into a "unique science". The rest of the brochure then summarily contradicts these two paragraphs by expanding the central theme which, printed in boldface letters, reads: "How Do Hydrologists Manage Water?" Significantly, the thesis that hydrologists manage water is taken for granted and the only matter considered worth talking about is how they do it. There follow prominent headings on Flood Damage Reduction; River Flow Management; Groundwater Management, Electric Power Production; Irrigation; Pollution Control; and Water Allocation and Planning, each outlining how the "hydrologist's skill" (not the science of hydrology) is used to help solve the related problems. Thus for example, we read that "to carry out these tasks the hydrologist uses an artful blend of engineering, biological and social skills"; that "to defeat [malaria and other waterborne diseases], it is the hydrologist's responsibility to work with health authorities in developing and applying effective remedies"; and that "it is perhaps in the field of reconciliation of the many conflicting demands for water that the hydrologist plays the most challenging role" (indeed, this role is at least as challenging as, for instance, would be the role of a land surveyor in a reconciliation of the conflicting territorial claims over Jerusalem). And, finally, to leave no doubt as to the purpose of hydrological research, the section on research makes things clear in its very first sentence: "Constantly trying to find better ways to manage water, hydrologists are involved in a wide variety of research projects".

Ironically, this last sentence rings of pure gold in capturing the essence of the main reason why such a vast amount of sterile research has been done in

hydrology: it has been directed at finding quick fixes to water management problems and, as a result, rather than contributing to hydrological knowledge, often resorted to shortcuts, avoiding the difficult hydrological issues and substituting unverified assumptions (embellished with fancy formalisms to give them respectability) for a hard search for the unknown facts.

The above example notwithstanding, the disease whose symptoms the ACH brochure so clearly displays is not endemic only to Canadian hydrology — it is a global plague. It has long been in evidence, for example, in the United States where 20 years ago, Yevjevich (1968) exposed the confusion between scientific truth and engineering expediency in connection with the Probable Maximum Flood; Hall (1971) criticized the common practice whereby "speculative assertions . . . become scientific fact . . . [by a] proper number of citations in the literature" and warned that "false concepts do not solve real problems — they merely create more. . . While we may be able to take stop-gap measures, . . . we cannot serve society honestly until hydrology is researched, understood and taught as a single system."

One of the strongest stands on this fundamental issue was taken by Rogers (1983) in his review of a book on the state of the art of models for environmental quality management, developed by hydrologists and others to accommodate the requirements of the [U.S.] National Environmental Protection Act. Undoubtably the legislators and the responsible managers were (to paraphrase the memorable pronouncement of the Canadian ACH) constantly trying to find better ways to manage hazardous substances in the water and air environment. But, according to Rogers, the result was that the models, themselves, were "hazardous to use. . . for practical applications or policy decisions" because "the engineering and scientific community [were] expected to perform analyses and prediction without a proper scientific base". He then asked the crucial question which should be printed in bold face above every scientist's desk: "*Why [does] the scientific community not refuse to collaborate with requests that [are] patently impossible?*" His answer is that many scientists eventually yield to the administrative and legal requirements and "[try] to do the best they can under the situation. In retrospect, this was a great error".

All these and many other voices have remained just voices in the wilderness. For the most part, hydrologists are still dancing to the tune of "constantly trying to find better ways to manage water", which is not theirs but technologists' task, and are either unable or unwilling to say "Enough is enough — our task is constantly to seek better solutions to the water balance equation" a mission spelled out clearly in the keynote lecture of Dooge (1987) at a recent international symposium.

It is true that, as Rogers (1983) says in a more general context, the most common direct reason behind the poor shape of hydrology as a science is the pressure of water managers on hydrologists in their employ (and most hydrologists are employed by water managers) to develop immediately useful technological tools and not to venture too far into serious research. To a manager in charge of a technology — and all those capitalized "hydrologist's chal-

lenges" listed earlier are technologies par excellence — the very word *research* conjures up the sum total of the worse horrors that he may ever face: An activity which cannot be properly planned, controlled, scheduled, and which in the end may come to nothing! And this is no exaggeration — here it is from the horse's mouth: "[It is necessary] to realize that nearly all scientific research leads nowhere — or, if it does lead somewhere then not in the direction it started off with" (you see, all the carefully planned priorities and objectives go up in smoke!) . . . "I reckon that for all the use it has been to science, about four-fifths of my time has been wasted, and I believe this to be the common lot of people who are not merely playing follow-my-leader in research." (Medawar, 1969). Well, if a Nobel Prize winner admits as much, it is not only prudent to keep ordinary scientists, such as hydrologists, on short leashes and constantly remind them not to get off the track?

However, universal and powerful as this obstacle is for hydrology on its route to the separation point past which the visions of the dictionaries may be transformed into a reality, it is not the most important one.

If hydrologists really wanted to do science and had strong ideas about what should be done, they could stand up and refuse to collaborate with requests that are patently impossible or unreasonable since the risks in doing so are minor compared to those that many people throughout history were willing to take when they believed in the rightness of their causes. Today there would be no hydrological Giordanos Brunos burning, with tied tongues, at iron stakes; not even many humiliated Galileos repenting on their knees.

So, why is it that hydrologists don't stand up? The main reason is that, after all, many hydrologists do not have strong feelings about the science of hydrology and are only too easily seduced into performing the technological pirouettes required of them — because, as discussed in detail on an earlier occasion (Klemeš, 1986), most of them are themselves technologists in their hearts. The thinking of the Canadian ACH criticized above is their own thinking and even though they may have moved into research from their original technological disciplines, subconsciously they still operate in the technologist's mode and are not displeased at all with the status quo. On the contrary, they are gratified that the clever shortcuts and stop-gap measures they are producing are regarded as science. And some of those who have made teaching their occupation have come to believe that these stop-gaps and empiricisms have become science by virtue of the fact that they started teaching them as professors.

I regard the phenomenon described in the preceding paragraph as the deepest root of the fact that hydrology is seldom practised as a science but rather as an unconnected set of techniques within different technologies; that "we have not one but many perspectives of hydrology" as J.C.I. Dooge pointed out in his lecture cited earlier and, which is worse, that the one perspective missing is — the hydrological perspective of hydrology. This also helps one to understand two important aspects which make it difficult for hydrology to establish itself as a science.

One of these is the conspicuous fact that so much of "hydrological research" is not "about water" but about various games of mathematical analysis which do not have much relevance or usefulness, either in the scientific or in the technological context. What else should it be about, given the typical makeup of a researcher working in hydrology — a civil engineer, a forester, an agronomist?

Suspended between a technology he does not practise and a science for which he has not been trained, his "research" is naturally guided to performing elaborate pirouettes on the high wire of techniques connecting the two distant poles and holding him in place. Seeing the long distance separating him from both of them, he clings to the wire and before long becomes an expert in random number generation, nonparametric optimization of parameters, second-order convergence of kriged Thiessen polygons, and other life (or face?) saving swings and displays.

The second aspect is even more damaging to the science of hydrology. The pressure to find immediate solutions to all the pressing water-related problems has created a vast body of hydrological *technology* which is mostly empirical and has very thin scientific underpinnings (a caveat sometimes made in hydrological textbooks — e.g. Linsley et al., 1975) but is often presented as a body of hydrological *science*. Since it offers "solutions" to almost any practical problem, hydrology is often regarded as a virtually closed science where not much more research is needed. For example, this was enunciated already 20 years ago at a symposium of Canadian government hydrologists by — of all people! — the then Chief of their Hydrology Research Division (F.I. Morton, pers. commun., on many an occasion between 1972 and 1986), and has freshly resurfaced in a document prepared by the staff of Water Resources Committee of the Science Council of Canada (1987): "Past research has largely resolved many problems in these areas [physical hydrology among them]. . . Scientists also know well enough [sic!] how to predict extreme droughts and floods and future water levels for optimum design and operation of dams and other river and lake control structures". Hence . . . "continued research into water quality, hydrology [among others]. . . should no longer be the main focus of Canada's overall research effort." Ironically, on the very next page, the impact of climate change on water supply, on Canada's water resources, transport of toxic chemicals through groundwater movement into surface waters are listed among the "most serious unresolved scientific issues".

The evidence documenting the fact that hydrology is regarded and practised as a technology rather than a geophysical science could fill many more pages, perhaps even volumes. Here I would just like to give a few seemingly trivial examples which, however, when taken as a whole, convey a clear and nontrivial message.

(1) The atmosphere of a recent Workshop on Evaporation and Evapotranspiration convened at the National Hydrological Research Centre in Saskatoon was succinctly described by the Centre's Director: "There was no strongly identified need to improve evaporation theory or models from the users. . .

Most seemed satisfied with the Meyer equation or similar" (T.M. Dick, intern. memo., June, 1987). Indeed, it seemed that the only outstanding problem in evaporation is the need for some minor local recalibrations of the coefficients in the Meyer equation.

(2) The American Geophysical Union (AGU) occasionally publishes a list of Index Terms for labelling Geophysical Abstracts (e.g. Eos, June 23, 1987, pp. 601 and 602). Hydrology is one of the 27 headings of geophysical disciplines listed. None of these list among their subheadings technological areas where they may be applied, except hydrology which lists Irrigation and Water Supply (Why not list Fishing under Oceanography; Wind Barriers under Meteorology and Atmospheric Dynamics; or Pendulum Clocks under Geodesy and Gravity?)

(3) The 1987 report on the Canadian activities within the International Union of Geodesy and Geophysics (IUGG) lists Water Resources Management as a prominent activity under the heading Hydrology, and gives, for instance, information on the installed capacity of Canadian hydropower plants. On the other hand, it was not felt necessary to list, for example, the tonnage of the Canadian navy under Oceanography.

(4) Worldwide, most of university teaching and research in hydrology is done by technological departments like Civil Engineering, Agricultural Engineering or Environmental Engineering (20 years ago, this author was teaching hydrology in Mechanical Engineering department).

(5) Granting institutions usually include hydrology among the engineering sciences.

(6) It is typical that hydrology research establishments build up their images by stressing their technological rather than scientific accomplishments.

(7) Research projects in most other geophysical and other sciences typically state what help will be needed from other sciences and technologies to reach their research objective. In hydrology, on the contrary, it must be stated how the research will help or serve somebody else's objectives.

(8) Should water be found on other planets, or on some other bodies in the universe, research on their water balance would most likely be enthusiastically supported and the only justification necessary would be that it might help to understand the water balance on the Earth. However, this justification is usually considered inadequate when research on the terrestrial water balance itself is proposed. Then prospects for practical applications have to be detailed, relevance to current water management priorities proved, etc., and in most cases the research is not supported anyway, unless packaged under some technologically sounding title such as Impact of Water Balance on Energy Production, or similar.

An outstanding attempt to help hydrology establish itself as a science was made 66 years ago when a few men of vision founded the International Branch of Scientific Hydrology within the International Union of Geodesy and Geophysics, later to be called the International Association of Hydrological

Sciences (IAHS). However, this good intention proved not to be enough, because of the general situation described in this section. The infrastructure from which the science of hydrology could grow was, and still is, sorely lacking. After all, most of those who were supposed to create this science were, and still are, not scientists themselves and only few were able to pull themselves up by their bootstraps and see that the technologies they were practising would, in the end, greatly benefit from a strong hydrological science. To their credit, many of the past IAHS Presidents belonged to these few as evidenced in the deeds and pronouncements of, for example, J. Smetana, O. Lütschg, J. Volker, J.C.I. Dooge, W.C. Ackermann, G. Kovacs (e.g. see the 50th IAHS anniversary issue of *Hydrological Science Bulletin*, 1972).

Despite the present sorry state of hydrological science, it seems, however, that its prospect is brighter today than it has been for years. It is so, because, among other things to be discussed later, the consequences of bad science are now increasingly more apparent and damaging to the very technologies which have so eagerly helped it to reach its present proportions. Some examples will be given in the next section.

FROM BAD SCIENCE TO BAD TECHNOLOGY

If need be, technology can do without science. If there is no science to be applied, technology must do its job regardless. It may be primitive, inadequate to meet the current needs of society, but this does not necessarily make it bad technology. For example, for a one-hour-ahead forecast of the stage on a large river, simply extrapolating the time chart of the stage by a straight line, or using its current reading, may be an adequate technology regardless of the amount of hydrological knowledge available.

History offers countless examples of excellent technologies produced without scientific knowledge and many of them come from the water management technologies, from the Sumerian irrigation systems of 4000 B.C., through the superb water supply systems of the Romans, to the elaborate Dutch drainage and land reclamation works, or the environmentally admirable networks of ponds and canals built in Bohemia in the 15th and 16th century.

Bad technology is one that applies bad science. Bad science is a science which presents unverified assumptions, untested theories, or arbitrary postulates as established facts; which makes claims that are unfalsifiable; which uses techniques whose sophistication is beyond the carrying capacity of the substance; which believes that an application of results of some other science or some scientific technique will automatically bestow a scientific status to its own results; which presents a piece of an ad hoc technology as an element of science.

Hydrology has often been guilty of all these transgressions, some of which will be illustrated in the following examples.

Deterministic hydrological models

The so-called hydrological conceptual models have been conceived in the spirit of good science, trying to improve on the black box models by introducing hydrological mechanisms and processes into the modelling of rainfall-runoff relations. The problem was that most of these mechanisms were not well understood; nor were their interactions, either in general or in specific conditions of different river basins. Thus the resulting models are concoctions of a few facts and many artificial constructions, in which the individual processes are represented by postulated rather than by established mechanisms and interactions and by assumptions arrived at by simplistic reasoning rather than serious hydrologic research. In effect they are, for the most part, just complex assemblages of black or, at best, dark grey boxes.

One revealing example of this fact can be found in an intercomparison study [World Meteorological Organization (WMO), 1975] in which the performances of several conceptual models were tested on the same sets of calibration and validation data. As a last minute entry, a model developed by Natale and Todini (WMO, 1975, Annex III j), though not a conceptual model, was also included. It is a simple linear black box model, modified by a physically based assumption that the response function must be constrained to allow only positive ordinates. As it turned out, this model performed on the whole at least equally well as the other models purporting to contain a much greater amount of hydrological science. Apparently, their complex structures and large numbers of parameters were far beyond the carrying capacity of the information content of the data used, of the amount of the hydrological substance of the models, or both. A similar result emerged in a later intercomparison study of snowmelt models (WMO, 1986) where a simple physically based empirical model developed by Martinec (WMO, 1986, Annex III. 25) easily matched the performance of conceptual models with various complex structures.

An interesting study which sheds some light on this situation involves a sensitivity analysis of one conceptual model involving thirteen optimizable parameters (Mein and Brown, 1978). When all thirteen were properly optimized as required, the modelled streamflows accounted for 95.3% of the variability of the measured data; when only three were optimized and the other ten set "arbitrarily high or to zero [or] to values which can be physically justified", the results accounted for 93.6% of the prototype variability.

It is thus more than likely that the exterior scientific appearance of most of the current conceptual models is just a smoke screen hiding an overkill of spurious epicycles labeled with respectably sounding hydrologic names.

A rare insight into the matter was recently offered by M. Sugawara, the author of one of the simpler conceptual models, known as the Tank Model, in a lecture at the National Hydrology Research Institute in Saskatoon (Sugawara, pers. commun., August, 1987). With his typical humility and sincerity, he told the audience about the practical reasons why his model represented a basin by four tanks: On the one hand, it would have been difficult

to handle more with the computing technology he used when he started developing the model some 30 years ago; on the other hand, four tanks was about the maximum for which he could still visualize the internal working of the system and relate it to the features of the output. With more elements he would not be able to do it and would be at the mercy of the model, so to speak. Next, for example in basins extending over a wide range of altitude causing a gradual melting of the snow cover, it seemed reasonable to divide the basin area vertically into several layers — so as to take account of the vertical temperature lapse rate driving the snow melt. The four tanks could be readily used as conceptual representations of these layers. Based on this rationale, he said, the model could always be fitted to work quite well. But, he continued, recently, he was asked to fit the model to one flat Canadian basin where it made no sense to visualize four different vertical layers.

"So", said the speaker, "I divided the basin into four parts horizontally and the result was good". When I asked him whether this might not indicate that the essential reason for the success may have been the availability of just the right number of the degrees of freedom supplying the necessary flexibility for the fitting exercise, he paused for a few seconds and with a disarming honesty said, "Yes, I think so, . . . but I hope that there were some temperature differences between the four parts of the basin".

Dr. Sugawara belongs to the unfortunately small minority of hydrologists appreciating the difference between science and technology and, after more than 30 years of searching for an identification of his nonlinear tanks with

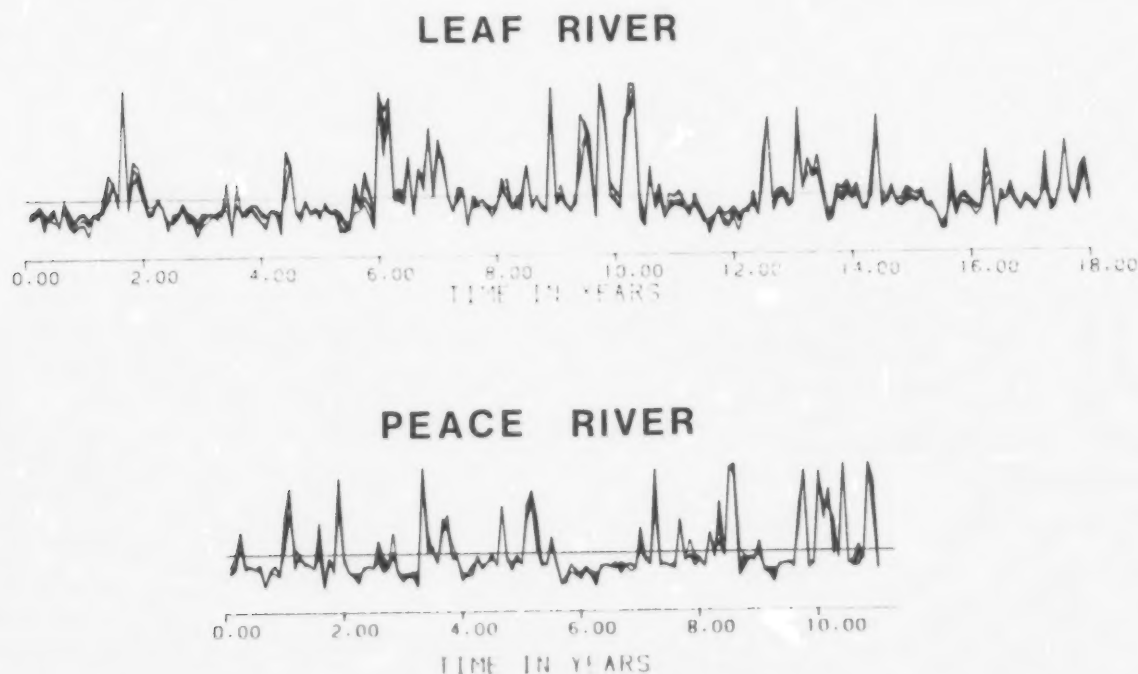


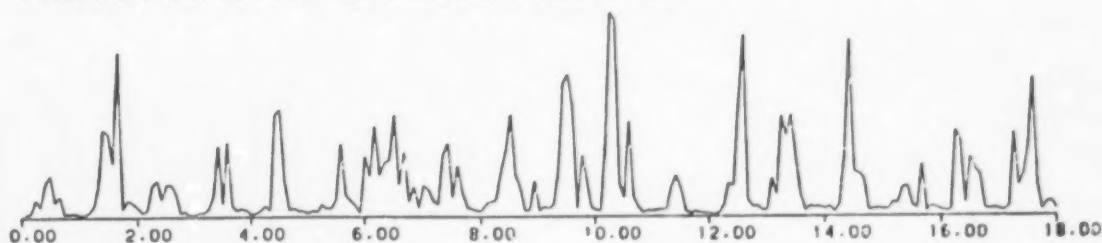
Fig. 1. Standardized series of simulated mean monthly streamflows, for twelve different climate scenarios (Klemeš, 1982).

specific physical parts of river basins, he still only has hopes that his technological tool may have a scientific value. In the majority of similar cases, such hopes are presented as established facts.

Taking the claims of the current conceptual models at their face value, and using them as reliable scientifically based technological tools leads to bad technology and may have serious practical consequences. The following example provides an illustration.

A widely used and believed to be scientifically sound "Sacramento" conceptual model was used to investigate the sensitivity of a water resource system to a hypothetical climate change (Némec and Schaake, 1982). The study

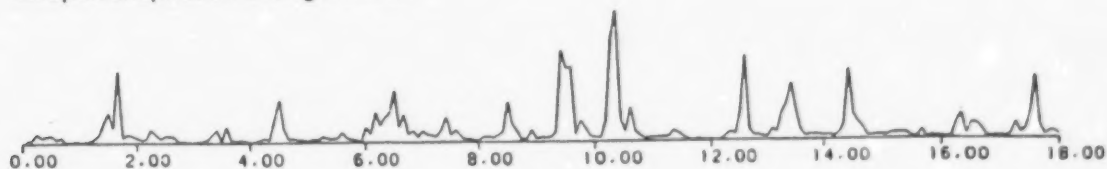
HISTORIC STREAMFLOW RECORD



SIMULATED DRY-CLIMATE STREAMFLOWS

Precipitation change -25%

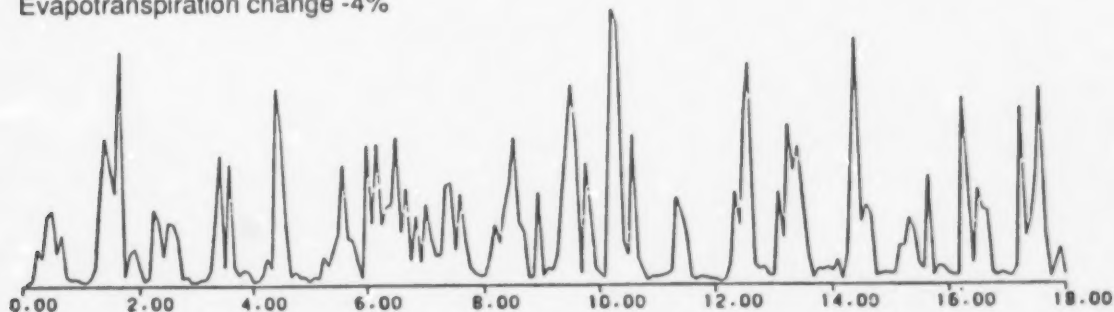
Evapotranspiration change +12%



SIMULATED WET-CLIMATE STREAMFLOWS

Precipitation change +25%

Evapotranspiration change -4%



TIME IN YEARS

Fig. 2. Mean monthly flows for the Leaf River and the corresponding simulated series for two extreme climate scenarios (Klemeš, 1982).

involved the simulation of stream-flow sequences in two basins for twelve different combinations of changes in historic precipitation and temperature which were fed into the model as forcing functions.

Based on the simulated streamflow sequences, water supply reliability from hypothetical storage reservoirs was then estimated. The exercise looked like a piece of a respectable water resource management technology, which it was supposed to be. However, the model, despite its presumably high scientific content and about twenty "physically based" parameters, all carefully optimized by calibration on the original historic records, proved inadequate for the given task. All twelve different streamflow sequences generated for each of the two basins collapsed into practically one and the same pattern given by the historical records, when standardized by the means and the standard deviations (Fig. 1). This indicates that the "hydrological sophistication" of the model was spurious since all it produced were practically linear transformations of the original streamflow records. The lack of hydrologic realism can be seen by closer inspection of the "wet" and "dry" flow sequences (an example is shown in Fig. 2) which retain the same lengths of dry periods between consecutive wet events, as well as the proportional magnitudes of the latter, irrespective of the necessarily different soil moisture conditions, infiltration, and evapotranspiration rates, water storage in the basin, etc.

Thus, as noted by Klemeš and Némec (1983) in a follow-up of the Némec and Schaake (1982) study, "with the present state of knowledge, and with the aid of the current tools reflecting it, only a very crude picture can be obtained whose usefulness to the planning process is extremely limited". Probably the only usefulness of the exercise (all its results are summarized in Klemeš, 1985), was in demonstrating the dangers of bad technology and in pointing to the need for genuine hydrologic research.

Probabilistic hydrological models

Notwithstanding its execution and results, in the development of deterministic models at least the program has been to increase the hydrological, i.e. scientific, content of these technological tools. Not so in the development of probabilistic hydrological models — quite the contrary, its program has been directed away from hydrology.

The rationale behind their development can roughly be summarized as follows: Hydrological science cannot predict future occurrences and magnitudes of floods and droughts, the sequential pattern of streamflow, or other hydrological phenomena which the technologist would like to know in order to be able to make well-founded water management decisions. Since these decisions cannot wait until the knowledge is available (there is no guarantee that it ever will), let's forget hydrology and let's just take the numbers that hydrologists have collected and treat them not as hydrological samples but as random samples from simple statistical populations, realizations of simple stochastic processes, etc. Thus, the problem has been redefined from the search

for hydrological interpretations of the patterns of hydrologic observations into fitting these into patterns of existing probabilistic models. This implies various assumptions that, in the hydrological context, are often arbitrary and sometimes inappropriate, since these models have been constructed for different purposes and with different aims.

Some of the developers and users of statistical models made it explicitly clear that these models did not have, and were not intended to have, any hydrological content (some of these disclaimers were cited in Klemeš, 1978) but were meant strictly as crude aids for water management decisions that had to be taken in the absence of hydrological knowledge, i.e. as elements of an admittedly primitive but helpful technology. In spite of these caveats, these models have been "elevated" to the status of hydrological science by virtue of their appearance in hydrological textbooks, being taught in hydrology courses and being polished in hydrological research literature. When returned to the users, from these pedestals of scientific establishment, they command the respect of a gospel not to be questioned but further embellished and practiced with a humble diligence.

Only two examples of the consequences will be given, randomly drawn from the recent additions to the deluge spilling over from the shelves of hydrological libraries. The references will not be cited since the examples serve only as *pars pro toto* and it would be unfair to direct attention to names whose bearers may belong more to the victims than to the villains.

The first example is taken from a work containing a large volume of useful hydrological information about an important river basin. The disasters occur when the author sacrifices his hydrologic judgment to "scientific theory".

Thus, in presenting an empirical cumulative frequency distribution of seasonal water level changes in a large lake (circles in Fig. 3), he offers a scientific interpretation of the data by fitting a Pearson Type III distribution

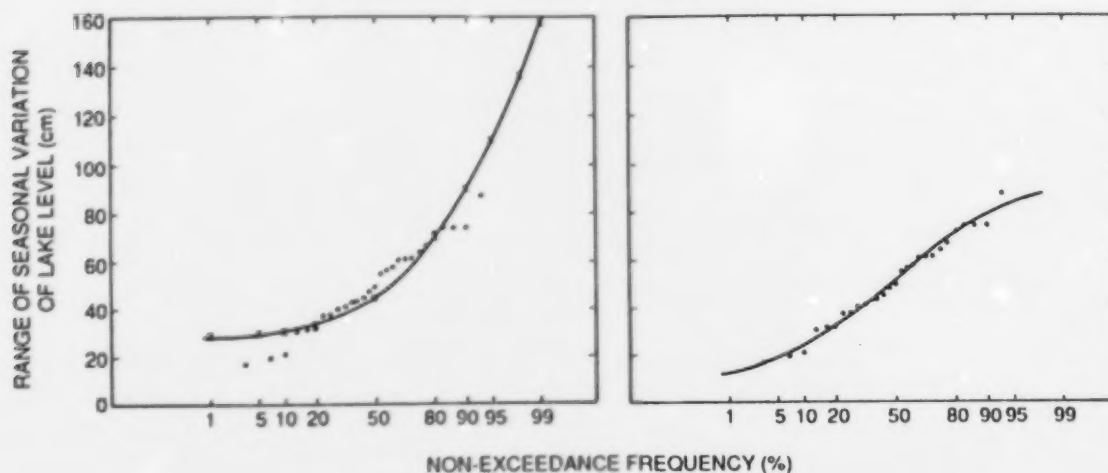


Fig. 3. Empirical distribution of annual ranges of water level fluctuations in a large and shallow equatorial lake, with the "best fit" of a "theoretical probability distribution" (left) and with a free hand fit (right).

(Fig. 3, left) which "... has proved to be the distribution of best fit" and from which he then derives "theoretical values of the range of variation... for a number of nonexceedance probabilities" (crosses on the line of "best fit"). Notwithstanding the automatic identification of frequencies within a 30-year historic period with probabilities supposed to be valid and equal in every single year (one of the main "scientific pillars" of all frequency analysis; see Klemeš, 1987 for more details), it is apparent from the description of the climatic, topographic and hydrologic conditions of the lake that its rise above some relatively low level would be extremely unlikely due to the vast shallow areas that would have to be flooded and the high evaporation which would ensue. Had the author not felt obliged to appear "scientifically up-to-date" and simply drew a curve through the data by hand (Fig. 3, right), his extrapolated nonexceedance "probabilities" may have been more probable than the "theoretical" ones.

While the preceding example does not go beyond bad science since no applications are made or suggested, the following one involves bad technology as well. It was published in a journal directed to water resource technologists and "... the goal of the development and use of the (Markov chain) model for snowfall is to predict the *most accurate* (emphasis added) probabilities for the change in annual snowfall in P."

The data base consisting of the record of annual snowfalls plotted in Fig. 4 (adapted from the original source) shows that, from about 1940, the snowfall was on the average lower than it was up to that time and that during the last 20 years it may have reached a new quasi-equilibrium state. This of course may be misleading and could be just a result of longer-term climate fluctuations; however, the decline also may be due to the effect of urbanization in the area (as the author, citing another source, pointed out). As much can be stated

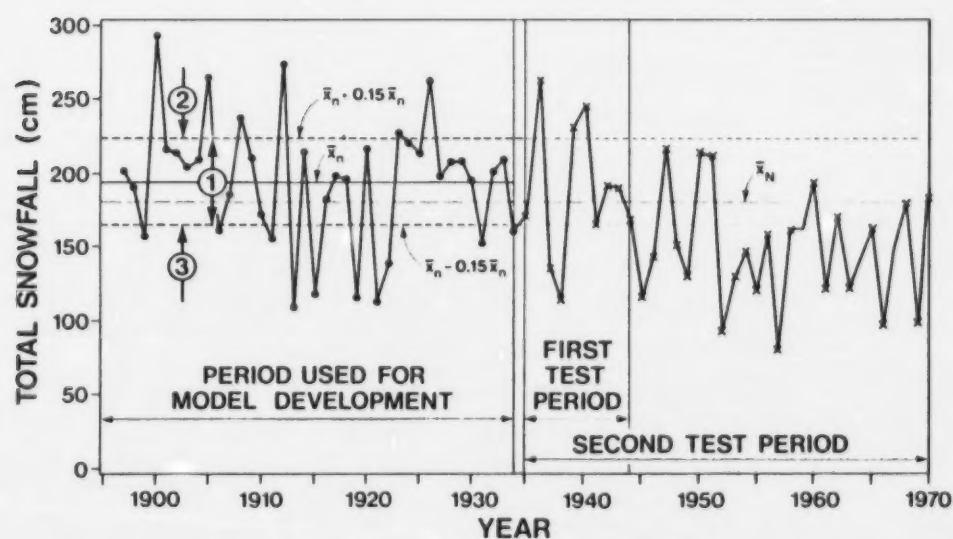


Fig. 4. Total annual snowfall at P; $\bar{x}_n$ = mean for period 1897-1934, $\bar{x}_N$ = long-term mean. Sample for 1897-1934 is split into three quantiles as follows: (2) $> 1.15 \bar{x}_n$ > (1) $> 0.85 \bar{x}_n$ > (3) (adapted from uncited source).

without involving any model at all. But this does not amount to much, especially in view of the stated goal of the paper, so hydrological science had to be brought into the picture, which is laudable.

The paper was authored by a student in a geography department, presumably under guidance of his hydrology professor. The most distressing thing about it is that neither the student nor his scientific counsellor, and not even the referee(s?) have noticed that the Markov chain models developed have actually not been used for the "forecast" at all. They were completely redundant to the whole exercise, although the elements of their transition probability matrices were computed "most accurately" indeed — to seven decimal places.

Rather arbitrarily, the period of the first $n = 34$ years was used for the development of the basic model. Its sample mean $\bar{x}_n$ was computed and three quantiles, 1, 2, 3, were delimited by boundaries $\bar{x}_n + 0.15 \bar{x}_n$ and $\bar{x}_n - 0.15 \bar{x}_n$, as indicated in Fig. 4. Then one-step transition probabilities between these quantiles were duly computed to seven decimal places from the empirical conditional frequencies (e.g. $p_{22} = 0.0000000$ is the probability that one large-snowfall year will be followed by another one). One would now expect these transition probabilities to be used for one-year-ahead forecasts in the two test periods selected. Not so. Instead, the transition probability matrix was gradually raised to higher and higher powers $a = 2, 3, \dots$, until for $a = 12$ it reached the inevitable stationary state where the conditional frequencies degenerate into the unconditional ones. These, however, can be found directly by simply counting the points in the three individual quantiles without any recourse to Markov, unless of course the actual aim is to make him turn in his grave. They are independent of the preceding states, so that by using them for prediction based on a given state, any information that this state may contribute is disregarded. This is exactly what the author did, thus defeating his original purpose stated as follows: "The Markov process was used over other methods. . . because the resulting model is dependent on the present state of the data. . . [and] therefore can be used as a predictor for snowfall amounts as well as an indicator of recent climatic change."

The test of the model consisted in comparing the three stationary quantile frequencies ("long-term probabilities") with those in the same quantiles in the following 10-year and 36-year periods. In the former case the similarity was found satisfactory, in the latter it wasn't, the reason being readily apparent from Fig. 4, but scientifically certified by chi-square tests. This then was the remarkable conclusion of the paper: "the model is a good predictor for changes in annual snowfall totals for up to 10 years after the model has been developed. . . [but]. . . would not be a good predictor of climatic change".

There is no need to examine the second model developed in the same way on the basis of the long term mean $\bar{x}_N$ except to note the final result: "In general, P. will not experience any unusual fluctuations in annual snowfall amounts in the near future. However, . . . a decrease in annual snowfall totals would seem plausible." The latter part of this conclusion is based on an observation that the frequency corresponding to the low quantile is slightly higher than that of

the high one, i.e. that the long-term empirical distribution has a slight positive skewness. This observation has truly global consequences; since most annual snow and precipitation distributions are positively skewed, a gradual decrease of precipitation world-wide seems inevitable.

If one wanted to be cynical one could say that the examples given above vindicate the opinions cited in the preceding section, i.e. that hydrology is a more-or-less closed science, that hydrologists know well enough how to predict floods and droughts and that the time has come to wind down hydrological research. Indeed, enough is enough and it might be better to shut up shop than to keep dealing in such science and technology.

The dangers of bad technology grow in proportion to the amount of its pseudoscientific content. An empirical technology openly admitting its own limitations is not dangerous. Of necessity, it will be applied with caution and it can be improved since it does not intimidate its user as does one endowed with an aura of "scientific nobility." It is easy to question, say, the concepts of the "rational formula" or the "runoff coefficient" but much more difficult, both technically and psychologically, to challenge a complex model, backed by references to the brightest stars of the hydrological science and by a 70-page listing of its computer program. It may even be that the development of bad technology via bad science i.e. via spurious "scientific" embroidery of dubious concepts, is sometimes done intentionally. For example, in criticizing water resource modelling technology, M.B Fiering (1976) said: "It seems clear that some models are pressed because professors are promoted and consultants are consulted in proportion to their generation of sophisticated, mathematically oriented models. This is too bad." Amen.

THE WATER BALANCE EQUATION

"The business of hydrology is to solve the water balance equation" proclaimed J.C.I. Dooge in his keynote lecture at the 1987 international symposium on "Water for the Future" mentioned earlier — a statement that said it all and showed once again the wisdom of the doyen of the science of hydrology. But there was no standing ovation and the spark of his polished and enthusiastic development of this central theme of hydrology did not ignite a flame of excitement in the audience. It even seemed that to most of those present it was something of an anticlimax. Didn't they come to hear about the great challenges of the future, about transforming deserts into green gardens, delivering humanity from floods and droughts, about the effect of climate change on the world's water resources? Has not the esteemed professor and statesman, the shining star of the international scene, been getting out of touch with the urgent tasks of contemporary hydrology?

Indeed, he has not. He poured a glass of a delicate pure wine and it was not his fault that many at the tasting party, addicted to the drinks of their "hydrological perspectives", could not appreciate the depth and elegance of its dry palate.

To hydrologists molded by pressures of their varied parent technologies to get fast results for all the urgent tasks of the current priorities, the water balance equation is a rather simple problem — you take care of your own pail of water and what is beyond are "losses." Thus for the hydrologist "managing water for Electric Power Production or River Flow Management" (to use the words of the Canadian ACH quoted earlier) only surface runoff counts on the debit side of the equation, evaporation and infiltration being "losses". On the other hand, surface runoff is a "loss" in the groundwater hydrologist's or agricultural hydrologist's equation, etc.

Only from one perspective — the hydrological perspective — is the water balance equation exciting in all its dynamics, with the competing forces of gravity, thermodynamics, chemistry, electrochemistry, and perhaps others, in the metamorphoses of one state of water into another as it moves through the hydrologic cycle, in the playful changes of its rhythm and pace over several orders of magnitude, in the structural complexity of the environments through which the water has to find its ways, and in the awesome complexity of the resulting patterns of its fluctuations in space and time. For the scientist, the water balance equation is one of the most challenging Rubik Cubes of nature, one in which the "squares" change colours, shapes and sizes as they are being moved around by the different forces, and in which even the structural setup changes with time.

But hydrologists have not been brought up to be motivated by curiosity and to solve nature's puzzles. To most, the water balance equation is never a beauty to be conquered but only a horse to be whipped to death to make it do its work; and it often seems that more dead horses are being whipped than live ones. In hydrological circles, including many hydrology research institutes supposedly set up with an explicit purpose to help unravel the puzzles of the water cycle, it is considered almost indecent to propose a research project motivated by curiosity and aimed at some fundamental hydrological problem, some puzzling aspect of the water balance equation.

WHY THE FUTURE OF HYDROLOGY HAS NEVER LOOKED BRIGHTER

There are at least three important reasons why the present era offers an extraordinary opportunity to fill the "vacant niche of hydrology as one of the geosciences" (Bras and Eagleson, 1987), namely: (1) the growth of the scale of human interference with nature; (2) the advances in observational and data handling technologies; and (3) the growing collaboration and interdependence between bio-geophysical sciences as they tackle problems on continental and global scales. A few words from a hydrological perspective about each of these aspects may be in order.

The growth of the scale of human interference with nature

Historically, hydrology has evolved from the practical needs of water-related technologies which, initially, had the following attributes. (1) They

were small-scale technologies — a well for water supply of a farm or a village; a dike along a stretch of a river; a weir to create a hydraulic head to drive the water wheel of a mill, etc. (2) They were concerned with only those elements of the water cycle which these technologies could control — hence the tendency to short-change those elements which were of no direct use to the technology in question. (3) The ranges of impacts on the water cycle of the individual projects were narrow; usually they didn't significantly overlap and the impacts did not interfere with one another. On this small scale, the individual technologies and their related "hydrological perspectives" were thus more or less independent and could follow their own paths. (4) The possibility of a small-scale water problem to be controlled relatively easily by technological devices did not give much incentive for an in-depth scientific hydrological enquiry: to get enough water for an isolated farmstead, an auger and good muscles were usually more needed than hydrology; it was similar with the draining of a swamp, creation of a few feet of hydraulic head needed for a mill, etc.

In short, small-scale problems whose solutions can be obtained by bringing them easily under control (I will call them "power solutions"), do not provide much incentive for the advancement of knowledge.

With an increasing scale of the problems, there is initially a tendency to increase the power of the solution rather than the knowledge base since it is well known that, to a certain extent, a lack of knowledge can be compensated for by an increased power (in this case by more wells, higher dikes, bigger dams, etc.). This often works until the individual "power solutions", once happily expanding far from each other, suddenly meet, clash, interfere with one another and, rather than providing solutions, start exacerbating the problems.

Many water technologies have now reached this point where the "power solutions" are becoming less effective and we are forced to shift emphasis from the traditional aim of *controlling* to the necessity to *adjust* to what apparently cannot be controlled — climate change, global ecological changes, population growth, the growth of scale of possible technological accidents, etc. This forced shift of emphasis puts a premium on knowledge and understanding, and is bound to change for the better the current attitudes toward the value of the science of hydrology and of solutions of the water balance equation.

The new observational and data handling technologies

When we look at the general scheme of the global water cycle (Fig. 5), we see that only a small fraction of the water fluxes are being and can be measured with available technologies. Ironically, the land-surface part of it, which is the domain of hydrology in the narrower sense, while having the weakest scientific basis and being most inadequately treated in the general circulation models, is relatively best endowed with observations. The problem is of course that it is only a very minor component of the global water cycle — the surface runoff — that can be most reliably measured, and even its observation coverage

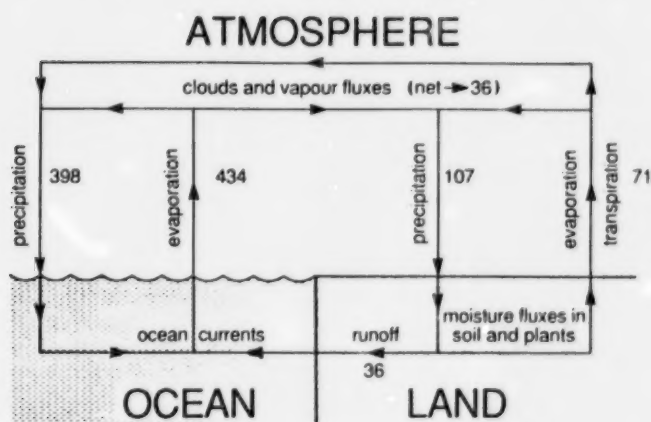


Fig. 5. Principal water fluxes of the hydrologic cycle in 10^3 km^3 per year; numbers taken from U.S. Comm. for IGBP (1986).

is far from adequate on the global scale. The second best covered is the precipitation on the land surface but again, its coverage on the global scale is very inadequate, with the aggravating circumstances of a limited representativeness of the standard point measurements for an areal distribution and of the fact that areas with the highest precipitation (e.g. high mountains) are least well covered with observation stations. The rest of the water fluxes are not and cannot be systematically measured in a direct way either because an effective technology is not available or an adequate network of the traditional point measurements is far beyond the resources available.

However, for the first time in history, there is a real hope for a radical improvement in the global coverage of hydrological (and of course other bio- and geophysical) observations.

It is the remote sensing technology which, while still of a very limited hydrological capability, holds great promise and has already yielded its first fruits, in the measurement of snow cover. Because the validity of solutions of the water balance equation on large scales can be verified only by the conduct of large-scale hydrological experiments, very much depends on the progress in the remote sensing technology, and on the development of new measurement technologies in general. In this sense, it is possible to state that hydrology research will be driven by technology. This is being stated with a grain of salt because hydrology has already had several rather disappointing experiences while being driven by various technologies. The results of its drive by the water management technologies has been discussed at length; another example is the computer technology which has driven many a hydrologist crazy and much of hydrology into a dead end of sterile manipulations of numbers through an explosion of "mathematical modelling" based neither on adequate data nor on adequate theory. To quote Fiering (1976) once more, "Fascination with automatic computation has encouraged a new set of mathematical formalisms simply because they now can be computed; we have not often enough asked

ourselves whether they ought to be computed or whether they make any difference" (compare the example on snowfall modelling in P. cited earlier).

Nevertheless, to be driven by a technology directed to the acquisition of the much needed data on hydrological processes seems to be free of the dangers connected with the other driving technologies mentioned since here the interests of the science of hydrology and of the new technology converge.

It is also hoped that the future availability of vast amounts of observations may give a new breath of life to the hydrological computing technology by providing it with something real to chew on — thus satiating its appetite for numbers and enhancing its present sterile diet consisting largely of computer-generated data like synthetic hydrographs, time series, simulated precipitation fields, river networks and other substitutes.

The coming era of bio- and geophysical sciences

The classical physics and chemistry owe their early successes to the fact that experiments of crucial theoretical importance could be done on a small scale in a modestly equipped small laboratory. However, further advances had to wait for the advancement of new technologies. Astronomy for its telescopes and space probes, particle physics for its city-size cyclotrons and power plants, chemistry for the heavy equipment to generate low temperatures, high pressures, vacua, etc.

Remote sensing, satellite, electronics and computer technologies have prepared the ground for advances in geophysical sciences by bringing within reach the practical possibility of conducting continent-scale and global-scale experiments without which the corresponding theories and models are toothless. Also, only on these large scales made accessible by the new technologies, is it possible to study the interactions between individual bio- and geophysical processes thus allowing the corresponding sciences to benefit from one another.

Glimpses of the role of hydrology in the future era of bio- and geophysical sciences could be caught at the recent XIXth General Assembly of the International Union of Geodesy and Geophysics. One could hear about the possibility of measuring the perturbations in the Earth's motion induced by the changing weight of continental ice and the seasonal distribution of snow; of using whole basins as "lysimetres" by recording pressure changes induced by the weight of basin water storage; of tracing the movement of evaporated water across continents and oceans; of studying the interactions between tectonic movements and groundwater; of the seasonal and spatial distribution of river runoff as an agent influencing ocean currents; of the processes in the tropical ocean initiating large-scale and long-term circulation patterns in the atmosphere, thus perhaps endowing some hydrologic phenomena with long-term memory and continuously changing the probabilities of their extremes, etc., etc.

Sadly, there is another powerful technological impetus for a close collabor-

ation and interaction between bio- and geophysical sciences. It comes from the integrated effects of the thousands of large and small useful technologies ranging from cars to fertilizers, which not so long ago were regarded as independent of each other and thus of little general consequence. However, the total is proving to be larger than the sum of the parts and its global effect is being revealed often with an alarming clarity and force.

The irregularly distributed, uncoordinated, and in their total uncontrollable and unpredictable millions of random elements of human civilization are now seen to lead to frightening deterministic global changes which it is up to the geophysicists, biophysicists and other natural and earth scientists to identify, interpret and predict. Without their knowledge, without significant advances in, and convergence of, their respective sciences, mankind will find itself in the dark, maybe heading to its own destruction through a diligent pursuit of the millions of its everyday "urgent needs and benefits." In this context, the water balance equation with "loss" terms has no place.

NEEDED: A CLIMATE CHANGE IN HYDROLOGY

If the present climate in hydrology persists, the opportunities that are opening up for its future can easily be missed. That does not necessarily mean that its business, i.e. the necessary solutions of the water balance equation, especially on large scales and in the various dynamic contexts, will not be attended to. It will. The only question is whether it will be done by hydrology or by other sciences which have recently started to court the water balance equation, solutions of which are now seen as increasingly important for deeper insights into many geophysical processes. It may well happen that, as the hydrology of the past and the present has been partitioned among the various technological disciplines endeavouring to *control* elements of the water cycle on a small scale, the hydrology of the future may well be partitioned among various geophysical and biophysical sciences endeavouring to *understand* the water cycle on a large scale.

Thus, instead of becoming a cohesive science with its own clear perspective, hydrology may be further fragmented and never rise above a vague concept existing only as a dictionary definition; and hydrologists, in their myopic pursuit of usefulness in their various small backyards may miss an opportunity to be useful to the world at large.

In order that this does not happen, a radical change in the hydrological climate is necessary. Its first prerequisite is an existence of a critical mass of hydrologists and water resource technologists who understand the difference between science and technology, substance and technique, advancement of knowledge and application of knowledge. I am confident that such a critical mass already does exist. There are many excellent scientists in the hydrological community and there also are many competent technologists who see the painful inadequacy of what is offered to them as hydrological science, who clearly realise that they don't need hydrologists to teach them how to fit curves

and extrapolate them, but to separate the hydrological wheat from the chaff and to grow more of the former. However this critical mass is thinly dispersed and dominated by a myopic majority which, because of the inertia of the past, controls the shaping of the future. It occupies the majority of the middle range of the decision making strata which are most important both in the distribution of resources downward to the working levels, and in controlling the flow of information upward to the powers that be which make the strategic decisions.

To change the present stifling climate, this critical mass must act in concert and not let itself be intimidated by the motherhoods and taboos of "priorities, client needs, mandates, relevance to resort objectives," etc. hurled upon them steadily from above. Above all, it must not let its ideas and information communicated upwards be castrated and filtered down to a state where their power is lost and they are no longer a serious match for the concepts they were meant to challenge.

Several lines of action can be pursued immediately:

Restructuring of hydrological education

A science cannot be built on technological training and education. This elementary fact must be recognized and acted upon. Here the responsibilities lie with the universities and their financial sponsors, be it governments or private bodies. If indeed hydrology is to become one of the geophysical sciences and its niche filled, it must grow out of the common core of geophysical sciences — physics, chemistry, geology and their specialized branches, and the curriculum must be built around the processes partaking in the dynamics of the water cycle. With its present structure of education of which perhaps 80% worldwide is rooted in technology rather than in science, hydrology cannot take advantage of the opportunities that the future is offering and it voluntarily abdicates from its legitimate position among the geophysical sciences.

Hydrologists trained in road construction, agricultural machinery, cost-benefit analysis, sewage disposal or logging operations can hardly be expected to contribute to an understanding of the elusive metamorphoses of water within the hydrologic cycle, to understand its role in the biosphere, its potential as a geomorphological agent, as the prime mover and redistributor of other chemical substances. With the present backgrounds and training of hydrologists it cannot be expected that even the few grains of genuine hydrological knowledge scattered throughout the vast amounts of publications will ever be brought together and molded into an internally coherent hydrological science.

Improvement in the teaching of hydrology for other disciplines

Engineering and science students are generally not dumb and don't deserve to be treated as if they were — which is exactly what usually happens in undergraduate hydrology courses. More often than not, these courses are

taught by staff for whom hydrology is a discipline of secondary or tertiary interest, or merely a nuisance subject that somebody has to teach, like it or not. In engineering courses, teachers usually rely on "applied hydrology" textbooks which, for the most part, are merely compendia of techniques designed not so much to apply hydrological science but rather to circumvent an absence of it. In science courses like geography, geology, ecology, etc., the students often get little more than reprocessed highschool material with an icing of "hard science" of flood frequency analysis and unit hydrograph, copied from the "applied" textbooks.

In both cases they should be taught hydrology as a geophysical science, told frankly what are the facts and what is the conjectural superstructure (Kisiel, 1971) and only then be instructed in the techniques that might be helpful to apply the knowledge or to mitigate against the ignorance. As it stands, they learn mostly how to apply the ignorance; they are offered indulgences for their future hydrologic sins.

When taught hydrology responsibly, as they are often taught mechanics, mathematics, chemistry, and other sciences, professionals in nonhydrological disciplines will not be deluded into thinking that an ability to derive a unit hydrograph makes one a hydrologist (ready "to manage water") as they don't think that solving an equation makes one a mathematician. On the other hand, they will have a good idea about what hydrology can do for their own technology or discipline and what they would like to get from the hydrological science.

Raising the standards of hydrological publications

The present trends must be changed from emphasis on quantity to emphasis on quality. In the end, the pursuit of quantity will defeat its basic underlying aim of increased economic gain for the publishers. Neither individuals nor corporations will be able and willing to invest in volumes where the substance is diluted below levels of recovery and usefulness.

This change requires not only higher standards of scientific refereeing in journals and dropping inappropriate criteria for acceptance of scientific communications at conferences, but also a much greater encouragement of scientific discussion both in journals and in other forums. An integral part of this encouragement is a necessity to increase the reward for the discussor for investing his time and effort. It should be brought to the attention of promotion, tenure, and other committees, that a discussion pointing out a flaw, or clarifying a point in a scientific publication, may be of more value to science than the publication itself.

Making IAHS the catalyst of the change

Who else should be expected to lead the way towards the establishment of hydrology as a vigorous and rigorous geophysical science than the only

international scientific association which claims the responsibility for the science of hydrology? Who else should bear more responsibility for the fact that "the cultivation of hydrology as a science per se has not occurred, and there has been no established platform within the hierarchy of science on which to build a coherent understanding of the global water cycle" (Bras and Eagleson, 1987) than an association which has proclaimed in its own statutes that its first objects are: "To promote the study of Hydrology as an aspect of the earth sciences and of water resources; to study the hydrological cycle on the earth and the waters of the continents, . . .?"

The IAHS has performed well in its other objects, in providing forums for presentation and publication of research results and in promoting the international aspects of hydrology, but it has greatly neglected its role in the scientific leadership. The scientific standard of its publications and conferences has not been better, and sometimes was worse, than that of other comparable ventures, its efforts to pull hydrology over the "separation point" between technology and science have left much to be desired and have been deservedly criticized by many hydrologists. This weakness has been recognized and, as the new President, I am determined to concentrate on this very aspect of IAHS activities. IAHS should and can become the platform for the change in climate in hydrology and with the help of the critical mass of hydrologists and technologists with a clear hydrological perspective, it will.

CONCLUSION

This paper has argued for the necessity of a clear hydrological perspective as a basis for the building of hydrology as a distinct geophysical science strong enough to bear its share of responsibility for the knowledge about the waters of this planet which has been aptly called the Water Planet. This hydrological perspective suggests that, rather than constantly justifying the practical relevance of fundamental hydrologic research, we start demanding a justification of the questioners' ignorance about it. Indeed, the time has come to ask the water resources establishments around the world "What have you done for hydrology?" rather than keep indulging their insatiable appetites for hydrological services which, at any rate, can only be faked without a solid scientific base.

ACKNOWLEDGEMENT

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